The Oxford Photographic Determinations of Stellar Parallax. Reply to Professor Turner. By Sir David Gill, K.C.B., F.R.S., His Majesty's Astronomer at the Cape of Good Hope.

Professor Turner, in meeting my criticisms of the Oxford parallax observations, unfortunately does not touch the principal grounds for doubting the reliability of the Oxford results.

First of all Professor Turner does not refer to Pritchard's fundamentally unsound assumption that, by the methods which Pritchard employed, it is admissible to give independent results for the parallax of the principal star relative to each of two opposite comparison stars. It is obvious that as the scale-value is derived from the distance between the comparison stars a and b, it must be assumed either that their distance $a\,b$ is constant, or that it varies proportionally to the time. But, if the parallaxes of the comparison stars are not identical, the distance between the stars will vary with the season of the year, and consequently the correction for scale-value (*i.e.* the corrected observed distances) will vary proportionally to this change. In other words, if the comparison stars are situated systematically with respect to the principal star, we should, by Pritchard's methods, obtain the parallax of the principal star relative to the mean of the parallaxes of the two comparison stars, but we have no means of distinguishing, as Pritchard attempted to do, between the parallax of the principal star as derived by measures from star a and those from star b.

The obvious answer to such criticism is—then, why not take the means of Pritchard's parallaxes from the stars a and b, and accept the result as the true parallax of the principal star relative to the mean parallaxes of the stars a and b ?

The answer to this question is given in a part of my criticism which Professor Turner omits to quote, and it runs as follows :—

" Although Pritchard frequently vaunts the novelty of his method, he takes no precaution to test its systematic accuracy. By simply taking photographs in the same season of the year at widely different hour-angles he might readily have ascertained whether the *apparent* parallaxes which he derived from observations six months apart, were really, in whole or in part, a function of the hour-angle at which the observations were made. It is obvious, for example, that any displacement of the apparent centre of the star's disc by chromatic dispersion of the atmosphere will, especially in the case of a bright star, be recorded on the photographic plate, and will not be eliminated, as in the heliometer observations, by the observer's superposing the

similarly coloured parts of the spectrum under observation. But amongst all the numerous plates taken and measured by Pritchard's assistants, one can find no published account of the application of any such simple and direct test."

It is the more extraordinary that Professor Turner should have neglected to reply to this criticism, because he seems alive to such sources of error when he says :—

"There may be very good reasons for using different scale-values in different directions quite independent of any hypothesis about distortion of the film. I mention one or two, quite on my own responsibility, not arising out of anything I have seen in Pritchard's work. 1. If the mirror or lens with which the photographs are taken is not an accurate surface of revolution (either essentially or because of flexure), its curvature in different directions, and therefore its focal length, will vary. There are obvious reasons why mirrors should err in this way more than lenses ; and it is a curious fact that plates taken with mirrors *do* show some variation in scale in different directions—*e.g.* the photographs of *Eros* taken with the 30-inch mirror at Greenwich (*Monthly Notices*, vol. lix. p. 13). If this is a *vera causa* Pritchard's rule would be essentially correct."

Now, in Pritchard's parallax work, practically all the observations in which the parallax factor has the + sign are made at eastern hour-angles, and all in which that factor has the — sign (from the same star of comparison) in western hour-angles ; or *vice versa*. If then, in Professor Turner's opinion (as also in my own), the flexure of the mirror may play an important part (especially where an angle of a few hundredths of a second of arc embraces the whole quantity to be determined), surely it is not too much to suspect that the effect of such flexure of the mirror may be to displace star-images by quantities which are not strictly symmetrical with the optical axis or with the image of the central star. Thus, as the direction of gravity with respect to the supports of the mirror is very different at eastern and western hour-angles, it was surely necessary to use the obvious precaution of taking photographs at the same season of the year both east and west of the meridian, in order to ascertain whether the apparent displacement of the principal star was due in whole or in part to the effects of flexure in the mirror or to such other causes as I have already indicated.

But Professor Turner's paper reveals other sources of possible error of which no mention is made by Pritchard, and which no one could have foreseen without access to the details of the original observations. For example, he states "that, for the period June 23 to July 1, *when a different mirror was in use*," &c. (the italics are mine). It would be interesting to know if different mirrors were used on other occasions, and what pre-

cautions, if any, were taken to replace the original mirror precisely in the same position relatively to its supports ; these are points on which the published results are entirely silent, although they are of crucial importance in estimating the reliability of the results.

As to the figures given by Professor Turner, they apparently present, in the form in which he arranges them, a better agreement than my original inspection of Pritchard's tables led me to think. Unfortunately for the moment our copies of the Oxford observations are in the hands of the binder in England, and I am unable to examine them. But whatever the result of such a re-examination might be, it cannot alter the conclusion that no reliance can be placed on the results, because they afford no data which can serve to distinguish between the displacements caused by instrumental or refraction effects and those which may be due to parallax.

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Postscript.—Since the above was written my books have been returned from England, so that I am in a position to complete a reply to Professor Turner's note.

Professor Turner, having rearranged Pritchard's Table XIII., not in order of date but in order of the mean value of the two corrections, appeals to an agreement of the majority of the signs as an answer to my general criticism, viz. "that throughout the series there is little evidence of systematic agreement even in the signs of the scale-value corrections, and their average discordance is very much greater than the systematic error of observation assigned by Professor Pritchard."

The table in question gives, *not* the actual discordances between the measures, but the effect of a difference between the mean scale-value and the instantaneous scale-value as independently determined from measures of the diagonals $a\,b$ and $c\,d$ (for a distance $= 1000''$).

Where the instantaneous scale value is grossly different from the mean scale-value there is a perfect agreement of sign, for it is obvious that by focussing badly enough or in any way adopting a sufficiently erroneous mean scale-value, one could always secure a similarity of sign in the instantaneous scale-value corrections from the two diagonals. Even as matters stand we find that out of 87 pairs of scale-value corrections, the components of which should have the same sign, no less than 24 have opposite signs. To examine the matter more closely, let us take simply the differences between the instantaneous scale-values derived from the stars $a\,b$ and $c\,d$. These are given in the following table :

Difference of Scale-value (on a distance of 1000″) derived from measures of the diagonals a b and c d.

Values of $(a\,b - c\,d)$.

1886.			
May 30 + 0″306	Sept. 15 − 0″111	Dec. 2 − 0″050	Mar. 16 − ″013
June 1 + ·098	16 + ·166	4 − ·233	23 − 0·216
4 − ·306	17 − ·041	7 − ·048	27 + ·029
8 + ·183	18 + ·090	9 − ·147	Apr. 2 + ·035
15 − ·334	20 − ·056	14 − ·078	16 + ·402
16 + ·213	22 + ·170	16 − ·372	19 − ·290
23 + ·592	27 + ·098	24 − ·372	20 − ·226
24 + ·269	29 + ·079	1887.	25 − ·097
28 − ·060	30 + ·278	Jan. 5 − ·090	26 − ·083
30 + ·591	Oct. 2 − ·073	8 − ·112	29 − ·072
July 1 − ·171	6 − ·080	10 + ·009	30 − ·007
Aug. 20 − ·034	13 − ·004	12 − ·138	May 5 + ·005
24 + ·049	21 − ·050	20 − ·044	7 + ·047
26 − ·124	22 − ·036	25 − ·022	9 + ·002
28 + ·274	Nov. 3 − ·034	31 − ·094	10 + ·071
29 + ·040	5 + ·087	Feb. 5 − ·100	13 − ·065
30 + ·079	16 − ·173	8 + ·071	14 − ·176
31 + ·014	17 + ·066	17 − ·067	16 − ·329
Sept. 7 − ·283	18 + ·028	25 + ·054	18 − ·019
10 + ·158	23 − ·200	26 − ·060	20 − ·277
11 − ·074	29 + ·018	27 + ·133	26 − ·098
13 − ·233	Dec. 1 − ·004	Mar. 12 − ·097	31 − ·048

If the observations were perfect all the quantities of the above table would be zero ; let us examine and see how far the errors of the table correspond with Pritchard's estimate of the accidental errors of his observations, or how far they contain other sources of error.

Pritchard's derived values of the probable accidental error of observation are the following :—

For Star a to 61_1 Cygni ± 0″091 (p. 17)

    ,, a ,, 61_2 ,,   ± ·100 (p. 24)

    ,, b ,, 61_1 ,,   ± ·115 (p. 31)

    ,, b ,, 61_2 ,,   ± ·100 (p. 36)

    ,, c ,, 61_1 ,,   ± ·102 (p. 47)

    ,, c ,, 61_2 ,,   ± ·088 (p. 52)

    ,, d ,, 61_1 ,,   ± ·089 (p. 58)

    ,, d ,, 61_2 ,,   ± ·104 (p. 64)

Mean ± 0·100

Now the sum of the squares of our table of 87 differences is

$$2 \cdot 8082.$$

The *mean error* of a single difference between the scale-values derived from $a\,b$ and $c\,d$ is therefore

$$\sqrt{\frac{2 \cdot 8082}{87}} = \pm\, 0 \cdot''180$$

or its probable error $\pm\, 0''\cdot 120$.
But the distance $a\,b$ is $= 2382''$
$\qquad\qquad\quad c\,d \quad = 2066''$

hence the probable error of the discordance of the distances $a\,b$ and $c\,d$, if reduced by an instantaneous scale-value derived from observations of both pairs, would be

$$\pm\, 0 \cdot''120 \times 2 \cdot 224 = \pm\, 0 \cdot''267$$

or the probable error of the measure of the single distance $a\,b$ or $c\,d$ would be

$$\frac{0 \cdot''267}{\sqrt{2}} = \pm\, 0 \cdot''190.$$

Now why should this extraordinary difference exist between the probable error of a measure when, on the one hand, the scale-value is determined along a line nearly coincident with the direction of measurement, and when, on the other hand, the scale-value is determined by the mean of two standard distances at right angles to each other?

This fact is the more remarkable because the smaller probable error, $\pm\, 0''\cdot 10$, is derived from measures between two images, one of which is that of the large and less sharply defined disc of the principal star, whilst the larger probable error, $\pm\, 0''\cdot 190$, is derived from measures between the smaller and sharper discs of the comparison stars.

There is only one possible explanation, viz. that some additional fault or cause of error, other than that of accidental error of pointing, is introduced, whose probable effect is

$$\sqrt{0 \cdot 19^2 - 0 \cdot 10^2} = \pm\, 0''\cdot 162.$$

And yet Professor Turner writes as if this error were non-existent, for he remarks "that Sir David Gill sets up this imaginary fault and proceeds to explain it by an equally imaginary cause"!

It is hardly desirable to treat as "imaginary" sources of error by which the amount of purely unavoidable accidental error of pointing is nearly doubled. It seems preferable to endeavour to trace their origin to some cause.

It is all very well arbitrarily to dismiss distortion of the film in the way which Professor Turner says that Pritchard did, viz.

" because he did not believe in this distortion ; " that is a very simple way of glossing over a difficulty, but it is not a good plan for getting at the truth.   It is also very easy for Professor Turner to deny that distortion of the film can exist ; he gives no proof to that effect, but, strangely enough, he next proceeds to give an explanation of the fault which he had just before dismissed as " imaginary " as follows :—

1. "If the mirror or lens with which the photographs are taken is not an accurate surface of revolution (either essentially or because of flexure) its curvature in different directions, and therefore its focal length, will vary.   There are obvious reasons why mirrors should err in this way more than lenses ; and it is a curious fact that plates taken with mirrors *do* show some variation in scale in different directions—*e.g.* the photographs of *Eros* taken with the 30-inch mirror at Greenwich (*Monthly Notices,* lix. p. 13).   If this is a *vera causa* Pritchard's rule would be essentially correct."

2. "If the plate is not strictly normal to the axis of the telescope, there would be variation in scale in different directions.   But in this case Pritchard's rule would not be quite correct.   If, for instance, the twist was about the line joining $a\,b$, then the scale-value would remain unaltered for the direction $a\,b$, and would be altered in the direction $c\,d$; but the distance from c to the star might be increased, while that from the star to d was diminished."

3. "If the distances on the plate in the direction $a\,b$ were *measured on a different day* from those in the direction $c\,d$, then Pritchard's rule would be essentially correct.   The small variations would then depend partly on different expansions of the plate with temperature."

With regard to No. 1, Professor Turner apparently thinks that if, from any cause, the focal length of the mirror were different in two directions, the scale-value of a plate, normal to the optical axis of the mirror, would be different in these two directions.   This is a mistake, for if the star images are symmetrical the scale-value is defined by the distance of the sensitive surface of the plate from the mirror, and by that only, and the scale-value is quite independent of the focal length. The effect of flexure or dissymmetry of the mirror would only be to produce ill-defined and probably unsymmetrical images in both directions, in pointing on which, errors would be produced in the measures of the standard distances, but such errors would not be linearly proportional to the distance, and consequently Pritchard's rule in that case is incorrect.

With regard to No. 2, Professor Turner's conclusions here are perfectly correct, but unfortunately Pritchard has given no information as to the mode in which the plate-holder is supported or the process by which the plate was adjusted normal to the axis of the mirror.   If the mounting of the plate-holder was

reasonably rigid it is improbable that this adjustment would change sensibly between one night and the next for plates taken nearly at the same hour-angle, and yet large differences (far exceeding the probable errors of pointing) do occur in the scale-values derived from the two diagonals under these circumstances. Professor Turner's hypothesis fails to explain this.   On the other hand, when the instrument is reversed (as it was at opposite parallactic epochs), it is not impossible that the adjustment of the plane of the plate with respect to the axis may have been changed to a small extent by flexure of the support of the plate-holder.

There are long periods—*e.g.* December 1 to February 5—in which the difference of scale-values $a\,b - c\,d$ (with one small exception) have the same sign, and this may possibly be due to a temporary maladjustment of the plate to the normal plane during that period.   Unfortunately Pritchard's published results afford no mention of the dates when such adjustments (if any) were made, and no definite conclusions can be drawn.   It is clear, however, that if the plate was liable to such errors the resulting parallaxes would be quite unreliable.

With regard to No. 3, Professor Turner says that if the two diagonals $a\,b$ and $c\,d$ were measured on different days the variations between the scale-values derived from $a\,b$ and $c\,d$ might depend on the different expansions of the plate by temperature. That may be true if we could admit the possibility of sufficient change of temperature, but even then it is only partly true, because Professor Turner omits to take into account the expansion of the steel screw of the De la Rue Macro-Micrometer in terms of which the plates were measured.   Changes of temperature would therefore produce changes in measurement depending on the difference between the coefficients of expansion of steel and plate-glass.   The difference of these coefficients is

$$0.000003 \text{ per degree Centigrade,}$$

corresponding, on a distance of $2000''$ to $0''.006$ per degree C. Thus, to produce a difference equal to the probable value of the error which we seek to explain, viz. $\pm\,0''.19$, it would be necessary that the two diagonals should be measured at temperatures differing by $32°$ Centigrade $= 58°$ Fahrenheit.   Is Professor Turner accustomed to submit his assistants to changes of temperature of this character in the measuring room ?   Pritchard apparently was not, for he says (*Mem. R.A.S.* xlvii. p. 4), "It was also found that, within the range of the small variations of temperature under which the instrument is used, no correction is required within the limits of the error of observation."   On the same page Pritchard also states that "the temperature at the time of observation was noted," so that, although no records of these temperatures are published, Professor Turner can satisfy himself by inspection of the original records whether what Pritchard considers "a small variation of temperature" amounts to $58°$ Fahrenheit.   But even if such uncomfortable treatment was

meted out to Pritchard's assistants, it must be remembered that each result is the mean of the measurement of four plates, and therefore Professor Turner's hypothesis would require the still more improbable assumption that in the mean all the *a b* measures were made when the assistants were shivering at 32° Fahrenheit, and all the *c d* measures when they were perspiring at 90° on the same scale, or *vice versa*.

The inadequacy of all Professor Turner's explanations is thus sufficiently proved. I therefore still venture to think that my hypothesis of unequal distortion of the film in different directions is, to say the least of it, a more probable cause of the errors in question.

But, after all, such distortion of the film may, by an easy-minded astronomer, be treated as an accidental source of error in the mean of many plates, although such an assumption does not touch the main grounds of my distrust of the Oxford parallaxes, viz. that, for the reasons given in the first part of this paper, the published results afford no proof whatever that the apparent parallaxes determined by Pritchard are not, in part at least, a mere function of the hour-angle at which the observations were made.

In conclusion, I take advantage of this opportunity to correct a mistake which I have made in the Introduction to vol. viii. part II. of the *Annals of the Cape Observatory*. I have there stated (p. xv) :—

" A precision, at least equal to that of Heliometer observations, has been claimed for the photographic method of determining Stellar Parallaxes. But, apart from their systematic errors, this is very far from being the case in the Oxford measures, even if we accept Pritchard's own result for their probable error, viz. $\pm$ 0″·10 for the single observation.

" The fact is apparently overlooked, that, for the single distances of which the residuals are discussed at Oxford, the maximum parallax factor is 1, whereas in the difference of two opposite distances, as discussed in the modern Heliometer method, that factor is 2. In other words, if the mean of the squares of the Heliometer residuals was the same as that of the Oxford residuals, one Heliometer observation would have four times the weight of one Oxford observation. But we have found (page 144 B) that the probable error of one observation derived from Gill's residuals is $\pm$ 0″·071 as compared with $\pm$ 0″·100 for Oxford ; their corresponding weights with equal parallax factors would therefore be as 2 : 1. Thus, having regard to the weight of the corresponding parallax factors, it would require measures of *eight* Oxford plates – even supposing them free from systematic error—to have the same weight in the determination of parallax as one of Gill's Heliometer measures."

My mistake in this criticism is in having overlooked the fact

that each of Pritchard's results depends on four plates, and not on a single plate as is there assumed.

The amended conclusion of my criticism should therefore be that it requires measures of thirty-two Oxford plates—even if they were free from systematic error—to have the same weight in the determination of parallax as one Heliometer observation.

The Oxford Photographic Determinations of Stellar Parallax. Further Reply to Sir David Gill. By H. H. Turner, D.Sc., F.R.S., Savilian Professor.

Sir David Gill kindly sent me a copy of the above reply, and it may be convenient to have my rejoinder read to the Society at the same time as his paper.

In his volume on stellar parallax Sir David Gill gave three reasons for regarding the Oxford determinations as not "of proved value," which may be briefly stated as follows :

(1) That Pritchard gave separate results for parallax from two comparison stars a and b, although the distance $a\,b$ had been used to correct each measure for scale-value.

(2) The "chromatic dispersion" objection.

(3) That the figures published by Pritchard gave evidence of distortion of the film, sufficient to render the observations of small value.

In a former paper (*Monthly Notices*, lxi. 5) I considered the third objection only, for as it deals with an actual matter of fact it seemed to me by far the most important. Sir David Gill's reply is chiefly in his postscript, which I will consider presently.

As regards the first and second points which Sir David now pronounces the more important, I am again at variance with him. To the first he has himself supplied "the obvious answer." Surely there is no harm done in giving the results separately ? If they are not independent the mean can be taken as Sir David Gill says ; and to exhibit them separately has the advantage of showing that there is no numerical mistake, if nothing more.

As regards the second point (between which and the first Sir David Gill traces some connection which I fail to see), it is quite true that Pritchard made no experiments on the effect of chromatic dispersion. It is equally true that Sir David Gill had made none himself at that time, though he had also published parallax work.

The discussion about chromatic dispersion of the atmosphere has come up since then. Indeed, it was not until 1898, when Pritchard had been dead five years, that Sir David Gill initiated